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Theo A. F. Kuipers

**THE NON-STANDARD APPROACH TO CONFIRMATION
AND THE RAVENS PARADOXES
REPLY TO PATRICK MAHER**

Patrick Maher's (PM, for short) critical paper requires a long reply. His first main point is my non-standard approach to confirmation. The second deals with my notion of conditional deductive confirmation and its application to the ravens paradoxes. In the first part of this reply I defend the non-standard approach extensively in a non-dogmatic way. In the second part I defend the notion of conditional deductive confirmation and its application to both counterintuitive cases dealing with ravens, or rather with black and non-black non-ravens. I am happy to be able to conclude this reply with a survey of the main interesting observations that I learned from Maher's critical exposition.

The Non-Standard Approach, i.e. the Success Definition of Confirmation

On Section 1: Definition of Confirmation

In Section 1, Maher criticizes my success definition of confirmation in a way that demands either retreat or extensive defense. For the moment I opt for the latter. In the introduction to Part I of ICR (p. 15) I announce the three main non-standard aspects of the success definition: its "reversive" (although I did not use that term), its "inclusive" and its "pure" character. That is, it *reverses* the definiens clause from '*E* makes *H* more plausible' into '*H* makes *E* more plausible', it is *pure* in the sense that it is neutral in rewarding hypotheses of different plausibility for the same success, and it *includes* the possibility of confirming "*p*-zero" hypotheses (i.e. hypotheses with probability zero). I shall deal with these aspects in the reverse order. Whenever the distinction is not relevant, I move freely between qualitative and quantitative, i.e. probabilistic, formulations.

In: R. Festa, A. Aliseda and J. Peijnenburg (eds.), *Confirmation, Empirical Progress, and Truth Approximation* (Poznań Studies in the Philosophy of the Sciences and the Humanities, vol. 83), pp. 109-127. Amsterdam/New York, NY: Rodopi, 2005.

Let us start, though, with a relativization by quoting a passage from the introduction to the quantitative chapter in ICR (p. 44):

Moreover, as in the qualitative case, it will also become clear that there is not one “language of quantitative confirmation”, but several, e.g., pure and impure ones, inclusive and non-inclusive ones. As long as one uses the probability calculus, it does not matter which confirmation language one chooses, the only important point is to always make clear which one one has chosen. Although speaking of confirmation languages is hence more appropriate, we will accept the current practice of speaking of confirmation theories.

Unfortunately I did not elaborate the ‘as in the qualitative case’ in the qualitative chapter itself. But implicitly it is fairly clear in that chapter that I am well aware that there are also different “languages of *qualitative* confirmation,” and hence that, if one assumes *the* obvious qualitative plausibility “calculus,” viz. the one implied by the (quantitative) probability calculus, “the only important point is to always make clear which one one has chosen.” Hence, my defense of the non-standard approach must be seen against this non-dogmatic background. At the end of the following defense I even propose a kind of fusion between my non-standard approach and the pure version of the standard approach.

1. *Zero probabilities.* Maher is right in demanding attention for the fact that a main disadvantage of my approach seems to be that confirmation by “*p*-zero” evidence (i.e. evidence with probability zero) is indeed impossible. I should have paid explicit attention to this possible objection.

1.1. *Verifying p-zero evidence.* Let us therefore start with Maher’s *prima facie* very convincing example of a specific real value as evidence fitting into an interval hypothesis. Maher is right in speaking about verification in this case, but he also wants to see verification, in line with the standard approach, as an extreme, but proper, case of confirmation, which is indeed impossible from my perspective. Inherent in my approach, and hopefully radiating from my (qualitative/deductive) Confirmation Matrix (ICR, p. 22) and the (quantitative/probabilistic) Confirmation Square (ICR, p. 46), is that verification is at most an *improper* extreme kind of confirmation (see ICR, pp. 46-7). Hence I would indeed like to “deny that in this case there is any [proper] confirmation at all” (PM, p. 4). Instead, it is a straightforward case of verification, not at all made problematic by being due to *p*-zero evidence. In such a case of verification, *E* (logically) entails *H*, and there is nothing more to say about it. For example, whereas in the case of (proper) confirmation it is plausible to distinguish between deductive and non-deductive (i.e. probabilistic) confirmation, a similar distinction is not relevant for verification, nor for falsification for that matter; formally speaking, verification and

falsification are unproblematic qualifications. In other words, it is not verification but deductive confirmation that is an extreme proper case of confirmation; verification and deductive confirmation only go together when H and E are logically equivalent.

Historians are well aware of the fundamental distinction between verification and confirmation. In many cases they can just verify their hypotheses of interest. Consider hypotheses about the date and place of birth and death that may have been suggested by some previous evidence. Such hypotheses may subsequently just be verified (or falsified) by consulting the relevant civil records. Of course, such data may occasionally be doubted, but that holds for all types of evidence and will have to be accounted for by the appropriate type of “Jeffrey-conditionalization” or by “globalization,” see below. Moreover, if verification is impossible, e.g. a town’s civic records might have been lost, historians will of course search for merely confirming evidence. Occasionally this may lead to deductively confirming, but non-verifying, evidence. For example, a more global civic register may survive in the archives of the province or region, containing only the years of birth and death, but not the precise days, let alone the hours. The fact that in the suggested historical cases the evidence may have been assigned some non-zero probability is, of course, not relevant for our arguing for a fundamental distinction between (proper) confirmation and verification

1.2. *Non-verifying p -zero evidence.* As I describe myself (ICR, p. 45) in an example, there are cases of non-verifying p -zero evidence that leave room for defining a meaningful posterior probability $p(H/E)$, despite the fact that the standard definition is not applicable since $p(E) = 0$. The consequence is, as I should have remarked, that this makes confirmation possible in the standard sense but not in my sense, which is technically similar to the way in which my definition leaves room for confirmation of p -zero hypotheses and the standard one does not. However, there is a fundamental difference between the relevance of the counterexamples. When $p(E) = 0$ because E reports a particular real number out of a real interval, it is very likely that one should take measure errors into account or that the relevant parameter, e.g. length, just cannot be said to have such a unique value. For both reasons it is then plausible to rephrase the evidence in terms of a small interval of values, which might be called “globalization” of the evidence, in which case, of course, we get p -non-zero evidence and hence the problem disappears. To be sure, there are cases where this globalization is also possible when dealing with p -zero hypotheses. Take, for example, the hypothesis that a certain die is unbiased. In view of the fact that totally unbiased dice will not exist in the real world, we should assign that hypothesis zero probability. Of course, globalization to a 6-

tuple of very small intervals will make a non-zero assignment plausible. Maher seems to suggest this strategy by the claim “the evidence confirms that the hypothesis is close to the truth” (PM, p. 3).

However, in other at least as typical scientific cases this strategy does not make much sense. Consider Einstein’s (general) test implication of (at least a certain degree of) light bending when passing heavy objects. For a Newtonian who assigns probability one to Newton’s theory, this test implication might well receive probability zero. Hence, however unproblematic Eddington’s data might have been (which they were not, but that is another story), they would not confirm Einstein’s specific general test implication according to the standard approach. However, some kind of globalization of the hypothesis, whether or not in the “close to the truth” form, is here out of order. Although Einstein made a much more specific, quantitative prediction, the prediction mentioned is already of a qualitative very global nature, but it nevertheless captures the fundamental surprise and risk of his GTR. Hence, in contrast to the standard approach, according to my theory, a “half-open-minded” Newtonian can see the experimental results as confirming evidence for Einstein’s theory. If so, he may well see this as a good reason for a non-Bayesian move, viz. changing his prior distribution such that Einstein’s theory receives a positive probability, however small.

1.3. *The counterfactual strategy.* Some version of this move is also suggested by Maher when he states that one may say when a scientist talks about confirmation of a p -zero hypothesis from the standard point of view “that what the scientist said is not strictly true, although it is understandable why someone might say that” (PM, p. 3). Of course, this response is also available for his discussion of confirmation by p -zero evidence. More specifically, in both cases one might interpret his way of talking as some kind of counterfactual personal claim: “if I would have assigned non-zero probability, to the hypothesis respectively the evidence, then the evidence would confirm the hypothesis.”

2. *The second comparative principle.* The second main reason for the success definition applies already to the “normal case” of non-zero probabilities for hypotheses and evidence. Maher does not pay attention to my emphasis on comparative principles. In this context, particularly P.2 (ICR, p. 24) and its generalization P.2G (ICR, p. 64) are important. Although P.2G does not entail the non-standard approach, I argue that it provides very good additional reasons for preferring the non-standard approach. Starting from the non-standard definition (SDC, ICR, p. 23):

- (a) E confirms H iff (E is a success of H in the sense that) H makes E more plausible

it is plausible to also have (see ICR, P.2G, p. 64, the core of which is):

- (Ca) E confirms H more than H^* iff H makes E more plausible than H^* does

E equally confirms H and H^* iff H and H^* make E equally plausible

The additional definitions for probabilistic versions of both subclauses are obvious: $p(E/H) > p(E/H^*)$ and $p(E/H) = p(E/H^*)$ respectively.

The standard definition:

- (b) E confirms H iff E makes H more plausible

suggests in contrast the “double” conditions:

- (Cb) E confirms H more than H^* iff E makes H “more more plausible” than H^*

E equally confirms H and H^* iff E makes H “equally more plausible” than H^*

which are not so easy to elaborate. In particular, for the probabilistic versions everything depends on whether one chooses the ratio or the difference measure as the degree of confirmation (or a close relative of one of them). Or, more cautiously, for judging “more more plausible” or “equally more plausible” one has to choose between comparing differences of the form $p(H/E) - p(H)$ or ratios of the form $p(H/E)/p(H)$. If one opts for comparing differences one’s comparative judgments come very much to depend on the prior probabilities of the hypotheses, my reason for writing in ICR of the impure nature of that approach to confirmation.

At least some philosophers of science seem to subscribe to (Ca), which only leaves room for the ratio measure (or a close relative). For instance, Elliott Sober (2000, p. 5) states the principle (in my symbols):

H is better supported than H^* by E iff $p(E/H) > p(E/H^*)$

See also (Sober, 2001, pp.30-3), where he calls a strong version of it “ E strongly favors H over H^* iff $p(E/H) \gg p(E/H^*)$ ” the Likelihood Principle. To be sure, Sober does not want to talk about ‘confirmation’ here: “We may ask whether an observation supports one hypothesis better than another. Here we’re not interested in whether the one hypothesis has a higher prior probability than the other; we want to isolate what the impact of the observation is” (Sober 2000, p. 5). Although many attempts have been made in the literature to draw such a distinction between confirmation and (evidential) support, I would like to argue that we might well read his principle in terms of confirmation. The reason is that I simply do not believe that scientists would not subscribe to the following general claim:

E better supports H than H^* iff E confirms H more than H^*

And I would claim, in addition, they have good reasons for that, for the only things that really count for the practical purposes of scientists are the unconditional and conditional plausibility or probability of evidence or, for that matter, of hypotheses, and their comparisons. Regarding “diachronic” comparisons, including comparisons of diachronic comparisons, it is rather unclear what other aim we can meaningfully have than “to isolate what the impact of the observation is,” that is, the pure perspective. Any other comparison will lead to a mixture of unconditional, conditional and “transitional” aspects, which can be decomposed into purely unconditional, conditional and transitional aspects.

To support this claim I consider cases of deductive and/or non-deductive confirmation of two hypotheses by the same evidence. The upshot will be that many intuitions not only suggest that the impure perspective is problematic, but also that a choice between pure and impure degrees of confirmation does not have to be made, and this only follows from the non-standard definition.

2.1. Comparing deductive confirmation. If both H and H^* entail E , they are equally confirmed according to (Ca), but according to (Cb) we have first to decide whether we want to compare ratios or differences. If we take ratios the same verdict results, but if we take differences we obtain that the resulting verdict totally depends on the relative initial plausibility of the hypothesis: the more plausible the more confirmed. It seems rather strange that for such essentially qualitative judgements one first has to make a choice between quantitative criteria. For example, both Newton and Einstein deductively predict the falling of stones near the surface of the moon. Would somebody who is told about confirming experiments by Neil Armstrong have first to make up his mind about whether he prefers comparing ratios or differences in order to judge whether one of the theories is more confirmed than the other or whether they are equally confirmed? If he were not to do so, he would consider this choice as irrelevant. But that would mean that he can’t subscribe to (Cb), for that requires a choice. On the other hand, if he wanted to make up his mind, he would be likely to subscribe to (Cb). If he then came to the conclusion that he would favor comparing differences rather than ratios he would in addition have to make up his mind about which hypothesis he finds the more plausible. On the other hand, if he prefers ratios he comes to the “equal confirmation” conclusion only by a rather technical detour. In sum, (Cb) forces one to consider technicalities of a kind that scientists, usually not very sympathetic to the concerns of philosophers of science, are not inclined to do. On the contrary, scientists are likely to have strong intuitions in the suggested case. In which direction, would essentially have to be tested by psychologists of

science, where the third possibility – more confirmation of the less plausible hypothesis – should also be taken into consideration.

2.2. *Comparing deductive and non-deductive confirmation.* Let us quote a long passage from Adam Morton's *Theory of Knowledge* (second edition, 1997, p. 186), with abbreviations between []-brackets added:

Evidence supports beliefs that make it more probable. Suppose a geologist defends a theory [H1] which predicts [P1] an earthquake somewhere on the Pacific coast of North America sometime in the next two years. Then if an earthquake occurs at a particular place and time [E1], the theory is somewhat supported. Suppose, on the other hand, that another geologist defends a theory [H2] which predicts [P2] an earthquake of force 5 on the Richter scale with its epicentre on the UCLA campus on 14 September (the anniversary of Carnap's death, incidentally) in the year 2,000. If this were to occur [E2], it would be very strong evidence for the theory.

In the following formalization I equate $E1$ with $E2 = P2$, neglecting the particular force, and indicate it just by E , because the force is not essential and $E1$ could have been any earthquake verifying $P1$. In this way we get:

$H1$ deductively predicts $P1$ hence, $1 = p(P1/H1)$
 $H2$ deductively predicts $P2 = E$ hence, $1 = p(E/H2) = p(P2/H2)$
 E logically entails $P1$ and is
 even much stronger hence, $p(P1) > p(E)$, $p(P1/H1) > p(E/H1)$
 E obtains

Morton, who, like Sober, also avoids talking about confirmation, concludes that E is very strong evidence for $H2$ and somewhat supports $H1$, but I do not hesitate to claim that scientists would see no problem in also saying:

$H2$ is more confirmed by E than $H1$

From (our formalization of) Morton's description it follows straightforwardly that $p(E/H2) = 1 > p(E/H1)$ and hence the case may well be seen as supporting (Ca).

But assume the (Cb)-perspective for a while. Of course, according to the ratio comparison we get the same verdict, for the denominator does not play a role: $p(E/H2)/p(E) = 1/p(E) > p(E/H1)/p(E)$. According to the difference measure this result obtains iff

$$p(H2) (p(E/H2)/p(E) - 1) > p(H1) (p(E/H1)/p(E) - 1)$$

and hence iff

$$p(H2) (1/p(E) - 1) > p(H1) (p(E/H1)/p(E) - 1)$$

which holds of course only under specific conditions. Let us assume that $p(H1) = np(H2) < 1$ and $p(E/H1) = mp(E) < 1$ then we get: iff

$P(E) > 1/(1+n(m-1))$). Although we may of course assume that m and n are both fairly large, such that the condition does impose a rather small lower bound for $p(E)$, it is nevertheless perfectly possible that $p(E)$ is smaller, in which case the opposite of Morton's intuition is satisfied: E confirms $H1$ more than $H2$. Suppose, for example, that $m = 11$ and $n = 100$, the lower bound is $1/1001$, i.e. 1 promille. Hence, the opposite situation certainly is a realistic possibility. In that case we would have a case where, at least according to Morton, " E is very strong evidence for $H2$ and somewhat supports $H1$," but philosophers of science in favor of the difference measure would nevertheless want to say that " E confirms $H1$ more than $H2$."

2.3 Comparing non-deductive confirmation. Let us now turn to the second example suggested by Morton (p. 186):

Or consider the hypotheses that a coin is fair [$H1$] and that it is biased [$H2$]. Suppose that the coin is tossed and [E] lands heads fourteen times and tails one time. This evidence is consistent with both hypotheses, but it has very low probability on the hypothesis that the coin is fair and much higher probability on the hypothesis that the coin is biased. So it gives much stronger evidence for the hypothesis that the coin is biased.

We may formalize the second example by:

$$0 < p(E/H1) < p(E/H2) < 1 \text{ and } E \text{ obtains}$$

According to Morton, E gives much stronger evidence for $H2$ than for $H1$. Again, I would not hesitate to claim that scientists would easily say, in agreement with (Ca) and, only, with the ratio version of (Cb):

E confirms $H2$ much more than $H1$

According to the difference comparison we get this iff

$$p(H2) (p(E/H2)/p(E) - 1) > p(H1) (p(E/H1)/p(E) - 1)$$

with perfect possibilities for an opposite verdict. Again we would have a case in which, at least according to Morton, E "gives much strong evidence for" $H2$ than $H1$, but philosophers of science favoring differences would nevertheless want to say that " E confirms $H1$ more than $H2$."

In sum, my impression is that scientists easily subscribe to (Ca). If they do that indirectly via (Cb) they need to be aware of a particular choice as degree of confirmation. However, since scientists usually do not express their attitudes in terms of degrees of confirmation (or support), it is more plausible that they directly subscribe to (Ca). Of course, this is my informal prejudice about the reasoning of scientists, which only a systematic research by interviews or questionnaires could decide. What "most works on confirmation theory" say (PM, p.2), is not decisive because the view of confirmation theorists may well be loaded by their favorite interpretation of what they think

about the way scientists reason. Of course, it is likely that I am myself a victim of this, but only meta-empirical research can decide on this. For some further elaboration of this point, see below.

3. *The reverse defeniens clause.* Maher claims that “According to etymology, dictionaries, and most works on confirmation theory, ‘ E confirms H ’ means that E makes H more plausible.” (PM, p. 2) Although I certainly agree that this “forward connotation” belongs to “confirmation” (vide my “reward principle”), I am not so sure that the “backward connotation” is not at least as important. My book presentation suggests that “forward confirmation” presupposes a success, that is, a success of a hypothesis is a necessary condition for confirmation of a hypothesis (by that same success). The question is whether it is a sufficient condition, of course, not according to “etymology, dictionaries, and most works on confirmation theory,” where the latter may be supposed to be written by philosophers (of science), but according to scientists. If so, the backward definition should be considered more basic. Maybe I should have presented my success definition not by (SDC, ICR, p. 23):

- (a) E confirms H iff (E is a success of H in the sense that) H makes E more plausible

as quoted by Maher, but in two steps, viz.

- (a1) E confirms H iff E is a success of H
- (a2) E is a success of H iff H makes E more plausible

which yields:

- (a) E confirms H iff H makes E more plausible

We should compare this with the “standard” interpretation:

- (b) E confirms H iff E makes H more plausible

which does not seem to be decomposable.

Neglecting extreme cases, I have the strong feeling that scientists will, when asked, agree with all four “absolute” claims as conceptually true statements. Above I argued that they are likely to also agree with the following comparative claims:

- (Ca) E confirms H more than H^* iff H makes E more plausible than H^* does
- E equally confirms H and H^* iff H and H^* make E equally plausible

rather than with

- (Cb) E confirms H more than H^* iff E makes H “more more plausible” than H^*

E equally confirms *H* and *H** iff *E* makes *H* “equally more plausible” than *H**

The reason is that if they subscribe primarily to (Cb) they not only have to assign some plausibilities or probabilities for any specific application, but they also in general have to decide about which degree of confirmation they prefer. This suggests that it is more likely that they combine (Ca) with the four absolute statements. Apart from the extreme cases, this can be perfectly realized by the ratio measure. To be precise, $p(E/H)/p(E)$ can deal with all five claims (that is, a1, a2 (hence a), b, Ca, and Cb), except when $p(E) = 0$. As I suggested in ICR (pp.50-1), for that case it is plausible to use the form $p(H/E)/p(H)$, which is for normal *p*-values equivalent to $p(E/H)/p(E)$ and which may be defined in this case, assuming that $p(H) > 0$. E.g. in the (improper) extreme case of verification, it becomes $1/p(H)$.

Taking my non-dogmatic attitude seriously, the result is that I could live perfectly happily with the following asymmetric fusion of intuitions:

- (ab) *E* confirms *H* iff
 - if *E* has some initial plausibility: *H* makes *E* more plausible
 - if *E* has none: *E* makes *H* more plausible
- E* is neutral for *H* iff
 - if *E* has some initial plausibility: *H* makes *E* neither more nor less plausible
 - if *E* has none: *E* makes *H* more nor less plausible
- (Cab) *E* confirms *H* more than *H** iff
 - if *E* has some initial plausibility: *H* makes *E* more plausible than *H** does
 - if *E* has none: *E* makes *H* more more plausible than *H** in the ratio sense
- E* equally confirms *H* and *H** iff
 - if *E* has some initial plausibility: *H* and *H** make *E* equally plausible
 - if *E* has none: *E* makes *H* and *H** equally plausible in the ratio sense

This completes my response to Maher’s Section 1.

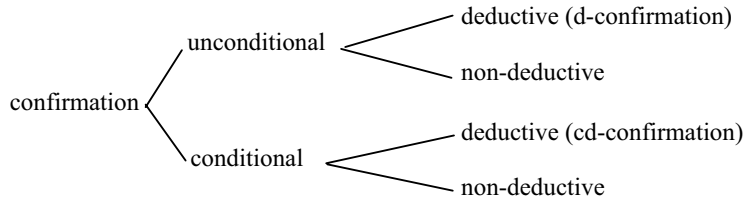
Conditional Deductive Confirmation and the Ravens Paradoxes

The rest of Maher’s paper deals with my notion of cd-confirmation (Section 2) and its application to the ravens paradoxes (Sections 3-5). Although he comes

up with a number of interesting formal observations, I reject most of his general critique in the following.

On Section 2: Conditional Deductive Confirmation⁸

In correspondence with Maher I have concluded that it is very important to stress that I see conditional deductive confirmation (cd-confirmation) as a kind of confirmation, but not as a kind of unconditional confirmation. More generally, the following distinctions in Ch. 2 and 3 of ICR are very important:



Before I go into a specific criticism of cd-confirmation, it may be helpful to refer to a sentence of Maher in the concluding Section 6 (PM, p.14). He writes: “C-confirmation [i.e., cd-confirmation with *C* as condition, TK] is just d-confirmation with *C* added to the background beliefs ... and so adds nothing essentially new.” I would not at all object to this claim. My only point is that in the context of cd-confirmation it is very practical to draw a distinction between fixed background beliefs and variable initial conditions as they occur as experimental conditions, such as, look for a raven (and then check its color) or look for a non-black object (and then check its (natural) kind). Note that in my formal presentations in ICR, I omitted, for simplicity, all references to fixed background beliefs, with explicitly saying so.

Let us now return to Section 2. Maher rightly suggests that I give two related but different definitions of cd-confirmation. However, from my presentation it is also clear that their relation is of a type-token or generic-specific kind. I start with (the general structure of) a token of cd-confirmation and then generalize it to a type. The token is of course basic, and there may be problems with the generalization. Maher summarizes both definitions in the second paragraph of Section 2 (with a correct simplification explained in Note 3). I repeat the generic definition of cd-confirmation:

⁸ Maher is right (PM, p. 5) regarding the desirable exclusion of “maximally plausible evidence” in the definition of deductive confirmation. When summarizing Ch. 2-4 of ICR for SiS (pp. 207-8), I discovered the omission that I had not made explicit that *E* is supposed to be contingent (and *H* consistent, in both cases relative to the fixed background beliefs), hence not maximally plausible. Note that my generic definition of cd-confirmation implies that *E* is non-tautological, by requiring that *C* is so, and *E* has to imply *C* according to that definition.

E cd-confirms H iff there exists a C such that E entails C and EC -confirms H

Maher successfully shows by THEOREM 1 that this generic definition is (still) defective in the sense that almost every E cd-confirms H . More precisely, he shows that when $LI(E, H)$ (i.e., E and H are logically independent) and when there is some D $LI(D, \neg E \ \& \ \neg H)$, then $C_{maher} = C_m = Ev(\neg H \& D)$ is such that E C_m -confirms H . For all conditions for specific cd-confirmation are now satisfied:

- (i) $LI(H, C_m)$
- (ii) C_m does not entail E
- (iii) $H \& C_m$ entails E

Of course, to prevent this trivialization one may either try to define specific cd-confirmation more restrictively or the generic type. In view of the very artificial nature of C_m it is plausible to first think of adapting the generic definition in order to exclude this C_m just because of its artificiality.

The condition $C_m = Ev(\neg H \ \& \ D)$ is in several respects not very like an initial condition as it occurs in standard examples of explanation of individual events or in my favorite examples of conditional confirmation, e.g. Cr : raven (x); Er : raven (x) and black (x). First, whatever E , H and D are, C_m can't be of a conjunctive nature, that is, an atomic formula or its negation or a conjunction of such formulas. Second, although C_m is logically independent of H in the straightforward sense, it needs H for its definition. Third, C_m needs D for its definition, although D is logically independent of $\neg E \ \& \ \neg H$. Of course, all three aspects can be used to exclude C_m . At the moment I would favor to requiring a conjunctive nature of C , but this may well be too restrictive and/or still leave room for other types of artificial conditions. However, Maher's counterexample does not at all show that it is impossible to prevent trivialization of generic cd-confirmation due to artificially construed conditions. On the contrary, it stimulates the search for an improvement of the generic definition.

On Section 3: The Ravens Paradox⁹

Regarding the object versus propositional form, it is evident that, for example, by giving an example of Nicod's criterion, i.e. Maher's PRINCIPLE 1, in object form, viz. 'a black raven confirms RH ', where RH is short for 'all ravens are black', the propositional form is the intended formal version of the more easy, but somewhat ambiguous, object form. Indeed, Hempel also

⁹ Unfortunately I speak about "raven paradoxes" and not of "ravens paradoxes". The mistake is due to the fact that in Dutch 'raven' is already the plural form (of 'raaf').

frequently uses the object form, but jumps to the other wherever relevant, and so do I.

More importantly, from my very brief indications in ICR (p.27) it is clear that I do not claim a new argument for the paradoxes. Hence, as far as the second paradox is concerned, I just intended to refer to Hempel's argument. Maher is certainly right in arguing that there is a gap between deriving (γ) and the claimed derivability of (β) from Nicod's condition and the equivalence condition. To argue, starting from (γ), that "any object which is either no raven or also black" confirms *RH*, in particular, a black non-raven, presupposes what might be called the "converse consequence property with respect to the evidence." This property is indeed problematic and, hence, not defended in ICR. In sum, Maher is right in claiming that Hempel's argument for deriving the second paradox is problematic.

Although I should have paid attention to this problem, my ultimate target would have been the same, namely to argue that, in the context of (conditional) deductive confirmation, a proper explication should not allow the confirmation of *RH* by (the proposition describing) a black non-raven. However, Maher also argues that this confirmation claim is not so counterintuitive as the one dealing with a non-black non-raven, i.e. the first paradox, whereas I suggest the opposite comparative intuition. Maher is certainly right in suggesting that there are contexts in which a black non-raven confirms *RH*. In my quantitative explication I concede this ((1*p*), ICR, p.59) when one is random sampling in the universe of objects, leading to the same degree of confirmation for all three cases. More generally, sampling, randomly or not so randomly, I would subscribe to both questioned confirmation claims as long as the sampling is not among non-ravens or black objects, that is, the context for (conditional) deductive confirmation. Unfortunately, Maher's formulations 'find[ing] a non-raven to be black' and 'finding a non-raven to be non-black' are in this respect rather unclear. In particular, I hesitate to subscribe to the reverse plausibility claim, but I do not exclude types of non-random sampling in which I would agree with this verdict.

On Section 4: Kuipers' Solution

In this section Maher addresses three points with respect to my solution of the first paradox of the ravens hypothesis *RH* (all ravens are black), which amounts to

- (4) a black raven *cd*-confirms *RH* more than a non-black non-raven

or to use Maher's preferred formulation

- (4) $Ra \ \& \ Ba$ *cd*-confirms *RH* more than $\neg Ra \ \& \ \neg Ba$ does

Moreover, the assumption is that the background beliefs (of course, the relevant ones, that is, *our* background beliefs) include or imply that the number of ravens $\#R$ is (much) smaller than the number of non-black objects $\#\neg B$.

Let us start with *Subsection* 4.3, where he claims that I should have written instead of (4):

(4') $Ra \ \& \ Ba$ Ra -confirms RH more than $\neg Ra \ \& \ \neg Ba$ $\neg Ba$ -confirms RH

However, it is very clear from the context of (4) that this is precisely what I mean more specifically. Let me just quote claim (2), starting just nine lines above (4), and even on the same page, viz. ICR, p. 28.

(2) a black raven and a non-black non-raven both cd -confirm RH , more specifically, a black raven on the condition of being a raven and a non-black non-raven on the condition of being non-black.

Hence I agree that, strictly speaking, (4') is my solution of the first paradox.

In *Subsection* 4.1 Maher points out, by THEOREM 2 (PM, pp.10-11), that the suggested *quantitative* rationale of the presupposed underlying *qualitative* conditional principle P.1c, unlike the unconditional version, is sensitive to the degree of confirmation chosen. That is, the ratio measure entails (in Maher's notation):

P.1cq: if E C -confirms H and E^* C^* -confirms H then
 $c(H, E/C) > c(H, E^*/C^*)$ iff $p(E^*/C^*) > p(E/C)$

but the difference measure does not.¹⁰ This is an interesting finding. But I am of course inclined to see it as an additional argument in favor of the choice for the ratio measure. Whereas the unconditional and the conditional quantitative version of P.2 are both in favor of the ratio measure, the unconditional quantitative version of P.1 is still satisfied by both measures. However, as soon as we consider the conditional quantitative version, i.e. P.1cq, only the ratio measure satisfies. Instead of seeing it as a case of circular reasoning, as Maher suggests, I see this conclusion more as a case of the so-called "wide reflective equilibrium" between qualitative and quantitative (and simplicity) considerations (cf. Thagard, 1988, adapting the ethical method developed by Rawls and Daniels). Sure, this does not provide a "rationally compelling justification" for P.1c, that is,

P.1c: if E C -confirms H and E^* C^* -confirms H then
 E C -confirms H more than E^* C^* -confirms H iff

¹⁰ Nor the likelihood ratio measure, which I neglect further, but the same points can be made for that measure. It may be true that Fitelson (2001) gives new arguments in favor of this measure, but in Kuipers (forthcoming, Section 1.2.1) I explain why his arguments in (Fitelson 1999) do not convince me.

E^* is, given C^* , more plausible than E , given C ,
in the light of the background beliefs

but I am happy with good reason. That is, the main question is, how plausible are P.1c and P.1cq? If they do not hold, there may be cases that a C -experiment is more risky for H than a C^* -experiment but the less surprising evidence E^* would nevertheless confirm H more than the more surprising E . Put in terms of ravens: although investigating ravens may be more risky than investigating non-black objects, RH could be more confirmed by hitting a (non-black) non-raven in the second case than by hitting a black raven in the first case. This sounds rather counterintuitive.

In Subsection 4.2 Maher shows, by THEOREM 3, that the ‘only if’ claim of my specification of P.1c, i.e., $S^\#$ 1.c, is not valid, using the ratio measure, for it turns out to leave room for a counterexample. However, I am not really impressed by the counterexample. It amounts to a case of “more confirmation by a black raven (assuming that it is a raven) than by a non-black non-raven (assuming that it is a non-black object)” even if the mentioned condition $\#R < \#\neg B$ is not satisfied in a straightforward sense. As Maher concludes himself, in the example “there is a probability 1/2 that $\#R > \#\neg B$,” but one should add that “there is also a probability 1/2 that $\#R < \#\neg B$.” Now it is easy to check in the example that the expected value of the ratio (of sizes, not to be confused with the ratio degree of confirmation) $\#R/\#\neg B$ is 7/8. Since this is less than 1 it is a nice case of a sophisticated version of the background belief that $\#R < \#\neg B$. That is, I would already be perfectly happy if all possible counterexamples nevertheless lead to a lower than 1 expectation for the ratio of the sizes. In other words, I would only be impressed, even very impressed, by an example in which this expected ratio is at least 1. In view of my earlier challenge to Maher to provide one, I conclude for the time being that he did not find one.

Maher also discusses the if-side of my claim $S^\#$ 1.c. With THEOREM 4 he points out that a sophisticated, probabilistic version of the if-claim obtains. However, I do not see what his objections are to my proof sketch on pp. 28-9 of ICR. I simply point out in terms of percentages that, whatever the numbers of the three types of individuals are that do not falsify RH , for every non-zero number of non-black ravens, hitting a black raven among the ravens is less plausible than hitting among the non-black objects at a non-black non-raven, as soon as the number of ravens is less than the number of non-black objects. This amounts to (II-R) in combination with (I), (III)-(V). Certainly, this is (at most) a case of quasi-quantitative reasoning that can only be made precise in a strictly quantitative sense, but that this is possible is very much suggested by the presented argument. Although I do not want to dispute Maher’s THEOREM 5, which is based on the general condition (II), I have only

claimed to subscribe to (II-R), in which the starred properties of (II) are limited to the complementary properties of the unstarred ones. For this reason, in contrast to Maher, I find it much easier to call (II-R) a plausible principle than (II). Finally, Maher is right in claiming that my proof sketch is strictly speaking laden with the assumption that RH is false. His proof of THEOREM 4 not only makes clear in detail that a quantitative refinement is indeed possible, but also that one only has to assume that RH is not certain.

On Section 5: Adequacy of the Solution

In the last substantial Section Maher mentions his criteria for an adequate solution of the (first) ravens paradox in terms of the three, inconsistent, principles mentioned in his Section 3:

PRINCIPLE 1: $Ra \ \& \ Ba$ confirms $(x) (Rx \rightarrow Bx)$ (RH)
i.e. an instance of Nicod's condition

PRINCIPLE 2 is the equivalence condition and

PRINCIPLE 3: $\neg Ra \ \& \ \neg Ba$ does not confirm RH

According to Maher, an adequate solution requires (a) identifying the false principle(s), (b) insight into why they are false, and (c) identifying a true principle that is sufficiently similar to each false one "that failure to distinguish the two might explain why the false principle is *prima facie* plausible."

These criteria sound very reasonable. Let me, therefore, instead of criticizing Maher's evaluation of my solution in detail, summarize my solution in terms of these requirements in combination with my basic distinctions. For it follows from my presentation, whether one likes it or not, that it is important to distinguish between deductive and non-deductive confirmation, and for each, between unconditional and conditional confirmation.

Starting with *unconditional deductive* confirmation, my diagnosis is that (a:) (only) the first principle is false, Nicod's condition, that (b:) it is false because RH does not deductively entail the purported confirming instance $Ra \ \& \ Ba$, and that (c:) " $Ra \ \& \ Ba$ Ra -confirms RH ," or equivalently, " $Ra \ \& \ Ba$ cd -confirms RH on the condition Ra ", is sufficiently similar to " $Ra \ \& \ Ba$ (d)-confirms RH " to explain "that failure to distinguish the two might explain why the false principle is *prima facie* plausible."

Turning to (specific) *conditional deductive* confirmation in general (a:) the third principle is false, because (b:) $RH \ \& \ \neg Ba$ entails $\neg Ra$, and (c:) which should be distinguished from the claim that RH entails $\neg Ba \ \& \ Ra$.

In terms of non-deductive, probabilistic confirmation, I claim (ICR, pp.59-60), assuming random sampling in the (finite) universe of objects, regarding *unconditional probabilistic* confirmation (ICR, p.59, (1p)) that

(a:) the third principle is false, that (b:) drawing any type of object compatible with *RH* is made more plausible/probable by *RH*, hence also a non-black non-raven, or, if you prefer the standard formulation: the probability of *RH*, if initially positive, increases by such evidence; hence the degree of confirmation for *RH* provided by a non-black non-raven is higher than 1 according to the ratio measure (and positive according to the difference measure), and that (c:) the ratio will be very close to 1: whether we calculate it on the basis of an estimate of the number of non-black ravens (if *RH* is false) or in the sophisticated way indicated in Note 19 of ICR (p. 59, p. 337), as long as the expected number of non-black ravens is a small proportion of the number of objects in the world.

Regarding *conditional probabilistic* confirmation, see (2*p*)-(4*p*) (ICR, pp. 59-60), everything becomes a quantitative version of the corresponding conditional deductive situation.

In sum, according to my analysis, in the unconditional deductive reading the first principle is false and the third true; in all other three readings the opposite is the case. In all four cases the verdict for each principle is explained. Finally, that the verdicts have to be reversed when going from the first reading to one of the other three explains very well why there has been a dispute and why it is so difficult to disentangle the purported paradox. In general: the truth-value of Nicod's condition depends on the precise version of the claim.

Let me finally deal with Note 7, in which Maher criticizes my quantitative treatment of the (first) raven paradox, without going into details. He just claims that the fact that a black raven confirms *RH* (unconditionally) is fallacious because this "is not true in Good's example." Now, in Good's example (Good, 1967), there are very specific and strong background knowledge beliefs. In particular, the number of black ravens is assumed to depend closely on whether or not *RH* is true: if *RH* is true there are 100 black ravens, and a million other birds; if *RH* is false, there are 1000 black ravens, one white, and again a million other birds. Of course, in that case a randomly drawn black raven should disconfirm *RH*, which it does according to all measures. But who wants to take this modeling as merely modeling random sampling in the universe of birds? One plausible way of modeling this, of course, is to assume that there is a fixed (finite, non-zero) number of black ravens and a fixed number of non-ravens, and some equally unknown finite but not necessarily non-zero number of non-black ravens, i.e., 0 or 1 or 2... My detailed unconditional claim (ICR, p. 59 and Note 19) is that when this modeling is adequate a black raven confirms *RH* (as well as a non-raven, black or non-black). For the moment I do not want to rule out that there are more defensible types of modeling random sampling among birds aiming at testing *RH*, but Good's case is not one of them. To put it differently, nobody would

see his hypothetical background beliefs as a normal type of case of not knowing the truth-value of RH . Of course, and this is Good's point, background beliefs around RH may be such that random sampling leads to the conclusion that a black raven disconfirms RH . On the other hand, the background beliefs around RH , other than those related to (relative) numbers, may be negligible, as I was apparently assuming, by not mentioning other kinds of fixed background beliefs.

Conclusion

This completes my response to Maher's Sections 2-5. In my comments on Section 2, I already referred in a positive sense to his diagnostic statement in his concluding Section 6 regarding the notion of conditional deductive confirmation. For the rest I have already pointed out that I do not agree with his conclusions. However, instead of repeating all disagreements, let me summarize the main interesting observations that I learned from Maher's critical exposition.

Section 1: taking my non-dogmatic attitude to confirmation seriously, I could live perfectly happily with an asymmetric fusion of non-standard and standard intuitions.

Section 2: the generic definition of cd-confirmation needs improvement, in view of THEOREM 1, to prevent it from trivialization.

Section 3: Hempel's derivation of the second ravens paradox is problematic, hence the question is whether it really is a paradox.

Section 4: THEOREM 2 shows that the difference measure for confirmation violates the plausible principle $P.1c(q)$, providing an extra reason for the ratio measure. THEOREM 3 suggests a possible refinement of the formulation of the number condition in my solution of the first ravens paradox: the background beliefs need only to imply that the expected ratio of the number of ravens to the number of non-black objects is (much) smaller than 1. But this should be checked, for both directions. THEOREM 4 shows that a similar weakening of the underlying assumption of the qualitative solution, viz. that the ravens hypothesis is false, is possible: the hypothesis is not certain.

Section 5: It is not yet generally realized that the truth-value of Nicod's condition very much depends on the precise version of the claim.

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